

Hooke, Newton, and the Trials of Historical Examination

The February 2004 issue of PHYSICS TODAY included a letter (page 13) in which Michael Nauenberg criticized my book *Meanest Foundations and Nobler Superstructures: Hooke, Newton and the Compounding of the Celestiall Motions of the Planetts* (Kluwer, 2002). The book is a study of a most consequential episode in the history of celestial mechanics: Robert Hooke's proposal to "compoun[d] the celestial motions of the planets of a direct motion by the tangent & an attractive motion towards a central body," which he submitted to Isaac Newton in a short, intense correspondence during the winter of 1679–80 to be realized in Newton's *Principia*.

The historiography of science has recently escaped somewhat the boundaries of academic esoterics to gain a new amateur readership; Nauenberg's letter represents an approach prevalent among that welcome new audience. Therefore, I thought it appropriate to point out some examples in which he misunderstands my arguments and the primary texts. A more detailed reply to his letter is forthcoming in the journal *Early Science and Medicine*.

My book analyzes Hooke's construction of his very original concept of bending heavenly rectilinear motion into a curved trajectory and demonstrates that Hooke developed his notion of "inflection" within the framework of practical optics. Nauenberg argues that "Hooke explicitly rejected the optical analogy [for] the '[b]lending' of the motion of planets." This is exactly the reason why I use terms like "construct" and "develop": There was no optical analogy for Hooke to discover and follow. Rather, "inflection" is a concept Hooke produced by purposeful manipulation of existing resources, utilizing aspects he found useful, such as the continuous change of direction, and discarding other aspects, like the reference to medium.

Nauenberg claims that I aver "without justification that the 'novelty of *De Motu* thus encapsulated [Newton's] willingness to represent forced motions by closed curves.'" On the contrary, my statement is a conclusion of a straightforward historical narrative (much of my chapter 3) in which I attempted to answer the following question: Why did Newton—and René

Descartes, Christiaan Huygens, and others—not develop a scheme of planetary motions similar to Hooke's, in which central attraction "inflects" the inertial rectilinear motion of the planet into a closed orbit?

When Hooke finally introduced his "Programme" in 1666, it was a complete novelty, and he struggled another 15 years before Newton or anyone else appreciated its significance. I discovered, to my surprise, that a closed curve orbit created by force had simply been inconceivable for even the most innovative of his

peers. Prior to Hooke's program, all models of planetary orbits (circulating slings, rolling balls, conical pendulums, and so forth) either assumed a circular cause—a rotating sun (Kepler for the planets) or turning hand (Descartes for the sling)—or simply posited a circular and force-free motion (Newton and Huygens).

It is here that my work distinguishes itself from Nauenberg's: The difference between what Newton should have realized and what he actually did, between what is formally trivial in hindsight and what was

self evident in his day, is the historian's starting point. Nauenberg is completely right in that Newton did develop "a sophisticated mathematical theory of orbital motion," but only in his 1684 *De Motu*. Time, for the historian, is of the essence. The cause must precede its effect, and the "sophisticated mathematical theory" and the description of orbital curves could have accounted for Newton's 1679–80 words to Hooke only if they were extant beforehand.

Nauenberg concludes by claiming that "both Hooke and Newton had a very similar and quite modern approach." "Modern approach," however, is hopelessly vague. If Nauenberg wishes to express empathy with the work of two 17th-century natural philosophers, that is commendable—provided one keeps in mind that Hooke and Newton were not attempting to meet our standards, but it is rather we who emulate theirs. If he means that Hooke and Newton were closer to us in their treatment of natural phenomena than most of their contemporaries were, he is right—that is what places them in the canon of the history of science. If he means that they were closer to us than to their contemporaries, he is

wrong: Hooke and Newton were 17th-century natural philosophers, and their interests, skills, motivations, approaches, and audience were of that era.

However, if Nauenberg simply means that Hooke's and Newton's ways of creating knowledge are more similar than different, he is gloriously right. Indeed, I am rather baffled by Nauenberg's mentioning "Gal's argument that Hooke's scientific style was 'radically different from Newton's.'" My book definitely contained no such argument and no such phrase. Here, precisely, is the main message of my book: that the works of the "genius mathematician" and the "ingenious technician" are similar in ways far more interesting than their differences. If that is all Nauenberg or any reader learns from *Meanest Foundations and Nobler Superstructures*, then, to quote Hooke one last time, "I am abundantly satisfied."

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Nauenberg replies: Ofer Gal writes that he discovered, to his

surprise, that "a closed curve orbit created by force had simply been inconceivable" to Isaac Newton before Newton finally learned about the idea from Robert Hooke. But ample historical evidence indicates that Gal's opinion is incorrect. For example, in a cryptic remark written in his notebook 15 years before his 1679 correspondence with Hooke, Newton stated that "if the body moved in an Ellipsis, then the force in each point . . . may be found."¹

In another manuscript, composed before his appointment as the Lucasian chair in mathematics in 1669 at Cambridge University, Newton found that "the force of gravity [at Earth's surface] is 4000 times and more greater than the endeavor of the Moon to recede from the Earth."² The discrepancy between Newton's figure and the correct value of approximately 3600 (according to the inverse square law) resulted from the erroneous estimate that he used for Earth's radius.

Apparently, Newton did not discover his error until shortly before starting to write the *Principia*. By applying Kepler's third law—that for the "primary planets the cubes of their distances from the Sun are re-

ciprocally as the square of the number of revolutions in a given time”—Newton had found that his apparently failed assumption that Earth’s gravitational force satisfies the inverse square law did apply to the gravitational force of the Sun. He wrote that “the endeavours [of the planets] of receding from the Sun will be reciprocally as the squares of the distances from the Sun.”³

By insisting that Newton did not develop a “sophisticated mathematical theory of orbital motion” before 1684, Gal indicates that he cannot understand the subtle mathematical results about orbital dynamics that Newton had exposed in his 13 December 1679 letter to Hooke. Acknowledging those results, however, would invalidate Gal’s arguments of what Newton learned about orbital dynamics from his correspondence with Hooke.

References

1. J. Herivel, *The Background to Newton’s Principia*, Clarendon Press, Oxford, England (1965), p. 130.
2. Ref. 1, p. 196.
3. Ref. 1, p. 197.

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Questioning the Rules in Coastal Erosion

We take issue with the PHYSICS TODAY article (February 2004, page 24) that praises the work of Keqi Zhang, Bruce Douglas, and Stephen Leatherman in documenting and promoting use of the Bruun rule to predict the impact of sea-level rise on shoreline erosion. We contend that the rule, a simple mathematical model,¹ has no basis in geologic or oceanographic reality but survives because of its simplicity, the lack of another approach, and a religious-like belief in the concept.

The Bruun rule doesn’t work in the context of our modern understanding of shoreface processes. For example, many shorefaces—the dynamic zones between the continental shelf and the beach—are not simply surfaces of sand but rather are underlain by rock or mud. In addition, sand-transporting bottom currents of many kinds occur on shorefaces and these are not considered in the model.

A particular absurdity of the rule is the assumption of a “sediment fence,” called closure depth, at the base of the shoreface; beyond that depth, significant amounts of sand

are assumed not to flow in a seaward direction. Ironically, as actually applied in coastal management, the Bruun rule reduces down to a single noninvolved variable: the slope of the shoreface.

We found¹ that the rule was being applied in at least 26 countries on six continents as a coastal management tool that adds a meaningless element to an already highly politicized process. The PHYSICS TODAY article does coastal management a major disservice by reporting favorably on a rule that doesn’t work.

Reference

1. O. H. Pilkey, J. A. G. Cooper, *Science* **303**, 1781 (2004).

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Douglas, Leatherman, and Zhang reply: Our paper investigated how increasing sea level will exacerbate the long-term sandy beach erosion that affects nearly 90% of the US coastline.¹ Storms are